

growth in *Tilletia*, *Ustilago*, and *Urocystis*. The synopsis of the Ustilagineæ in relation to their supporting plants and the place of their spore formation will be very useful to students, as will also the counter-synopsis of the supporting plants, and the Ustilagineæ occurring on them. The details of the germination of spores, direction and character of the promycelia, the effects of moisture, light, &c., measurements of threads and spores, all combine to render this a useful contribution to the literature of the Smuts, although not containing any startling discoveries. It is just what it professes to be, the record of observations on the germination of the spores of several of the Ustilagineæ under artificial cultivation, as a supplement to Tulasne's memoir in which this history of development was deficient. It would have been an advantage had this "Contribution" made its appearance in the trade as a separate publication with a London publisher, at a fixed price, so that all persons interested in the subject in this country might have obtained copies, and recommended the work to their mycological, horticultural, and agricultural friends.

M. C. C.

### LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.]

#### The Philippine Islands

ENCLOSED you will find:—I. A list of Earthquakes on the Philippine Islands from January to March 1872. As communication is very bad here, and meteorological observations are seldom made and noted down, I may say that most of the earthquakes do not come to our knowledge at all. I believe I do not say too much in expressing my opinion that there may be, at least, one earthquake every day at some one spot in this Archipelago.

2. Description of a Typhoon, which I witnessed at Cebu.

#### EARTHQUAKES ON THE PHILIPPINE ISLANDS

Since my last communication to the list of earthquakes to your journal (Feb. 5, 1872) I have noted the following:—

- |          |                                                   |                                                                                                                 |
|----------|---------------------------------------------------|-----------------------------------------------------------------------------------------------------------------|
| Jan. 27, | Zambales in Luzon, E.-W., many and strong shocks. |                                                                                                                 |
| Feb. 7,  | Camarines on Luzon, twice.                        |                                                                                                                 |
| Mar. 5,  | Manila, weak, 9 A.M.                              | } These two were perhaps on the same day, and a mistake has been made in the letter which announced the second. |
| 6,       | Province Laguna in Luzon, 9 A.M.                  |                                                                                                                 |
| 22,      | Manila, several very strong shocks.               |                                                                                                                 |
| „        | Province Batangas in Luzon.                       |                                                                                                                 |

#### TYPHOON AT CEBU

April 4, I witnessed a Typhoon in the harbour of Cebu, Philippine Islands, on board H.M.S. *Nassau*, Captain Chimmo. The following gives a short description of it, hoping that the officers of that ship will publish a detailed account of this interesting storm:—

| Barometer |       |                                                                                                                                                                                                                                                              |
|-----------|-------|--------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------------|
| 8h A.M.   | 29.96 | } Strong N.W. winds, heavy rains.                                                                                                                                                                                                                            |
| 2h P.M.   | 29.82 |                                                                                                                                                                                                                                                              |
| 3h 30m „  | 29.74 | } Wind, N.W. (4 to 8) rain.                                                                                                                                                                                                                                  |
| 6h „      | 29.54 |                                                                                                                                                                                                                                                              |
| 7h „      | 29.40 | } Wind and rain ceasing, till some heavy squalls from N.W., followed by perfect calm.                                                                                                                                                                        |
| 8h „      | 29.30 |                                                                                                                                                                                                                                                              |
| 8h 15m „  | 29.28 | } Lowest marking of the barometer, till most furious squall from S.E. (11) with heavy rains and lightning. This furious storm lasted about ten minutes, and then, varying from S.E. to S.W. (hardest from S.) diminished slightly; barometer rising rapidly. |
| 10h „     | 29.76 |                                                                                                                                                                                                                                                              |
| 12h „     | 29.86 | } Wind steadier, squalls less frequent, and heavy rain ceased.                                                                                                                                                                                               |
|           |       |                                                                                                                                                                                                                                                              |
|           |       | } Wind steady from S.S.E. (5) dying away till daylight.                                                                                                                                                                                                      |
|           |       |                                                                                                                                                                                                                                                              |

About eighteen vessels were thrown on shore, more or less damaged, many houses unroofed, and native huts blown away

throughout the island Cebu, and several lives lost. At Horlo in Panay the storm did a great deal of damage too; at Manila it was not observed at all. It is said that there must have been at the same time a storm at Sargoon. Typhoons are very rare as far south as Cebu, and are said not to have been observed for twenty years. This storm proves to be a real typhoon, according to the variation of the wind and the calm between it, showing that the centre passed Cuba.

Manila, April 15

ADOLF BERNHARD MEYER

### The Conservation of Energy not a Fact, but a Heresy of Science

PERMIT me a few words in reply to Mr. Brooke's strictures in your journal (No. 137) upon my article on "The Heresies of Science" in the *London Quarterly Review* of July last.

Mr. Brooke asserts that in the article "two widely different principles are oddly linked together as heretical dogmas, the doctrine of Evolution and the Conservation of Energy." Now, so far from these doctrines being oddly linked together as heresies, they are not linked at all. It is not the doctrine of Evolution, but the hypothesis of Natural Selection that I affirm to be one of the great heresies of modern science. Evolution is dealt with only so far as is found necessary to prove that the theory of Natural Selection is false. The two heresies named are connected in the article because I found so many physicists employing them to overthrow some of the best established truths in philosophy. Of this Mr. Brooke is perfectly aware, since he expresses regret that "the principle of the Conservation of Energy has by some been misapplied in a fruitless endeavour to supersede the necessity of a creative intelligence."

"The Conservation of Energy a Fact, not a Heresy of Science," is the title of Mr. Brooke's paper. To this assertion I need only oppose some of his own admissions. He complains that the proposition—viz., "that the amount of energy in the world is unchangeable, the sum of the actual or kinetic and potential energies being a constant quantity"—has been by some writers rather overstrained. "It may," he adds, "be taken as a postulate, and is probably true, but it is a proposition that is equally incapable of proof or of disproof, because the amount of potential energy in a body can be determined only by its development into actual energy, and cannot therefore be predicated."

Are then our judgments respecting that which Mr. Brooke asserts to be a fact of science only probably true? Surely there is contradiction here. I take it that science is knowledge, and that consequently judgments not accompanied by a conviction of certainty, but merely possessing a higher or lower degree of probability, are altogether outside the sphere of science. As Mr. Brooke accepts the doctrine of the Conservation of Energy as a truth of science, it is not competent for him to maintain that the proposition—viz., "that the amount of energy in the world is unchangeable, the sum of the actual or kinetic and potential energies being a constant quantity"—is equally incapable of proof or disproof, unless he can show that it expresses one of those primary convictions of the mind which constitute the very starting points of human thought. Let Mr. Brooke do this, and there is an end to all discussion on the subject. By one of the laws of thought a proposition which can neither be proved nor disproved, but by other propositions not more evident or more certain, must, by all rational minds, be accepted as true. In this region doubt becomes suicidal by self-contradiction. It is easy to show that the proposition which constitutes the scientific expression of the doctrine of the Conservation of Energy is not the symbol of a primary synthetical judgment. It is really nothing but a truism rendering to the scientific inquirer no higher service than the statement that "every effect must have a cause." In all such cases we grant the truth of the proposition when we grant the definition of either of its related terms. "That everything which begins to be has been produced, immediately or mediately by the power of an intelligent being," is the only philosophic expression of the great law of causation. Stated thus it becomes the appropriate symbol of a primary and necessary synthetical judgment of which every sane mind is conscious. No less a thinker than the late Sir John Herschel held that the doctrine of the Conservation of Energy is a mere truism. It is so as the result of the introduction of what he terms the unfortunate phrase "potential energy."

Mr. Brooke says that "energy is the power of doing work." He does not tell us what he means by work. If he means motion in any of its modes, then he confounds what he holds to

be distinct realities, viz., Force and Energy. Further, he states that "the term 'potential' applied to Force or Energy means inactive, but capable of being called into action. Thus, if a weight be raised, a certain amount of energy is expended in raising it, and so long as the body is supported the energy expended in raising it remains potential in it, but when allowed to fall freely *in vacuo* to the level from which it was raised, the body acquires exactly the amount of energy that was expended in raising it." This too is the view of Tyndall and Balfour Stewart. According to this doctrine, if I throw a stone upwards, say to the height of twenty feet, the energy expended is not lost, but gradually changed in form as the stone ascends. When the stone leaves the hand its energy is actual, at its maximum height it is potential. As a form of potential energy it is a kind of power existent in the stone, but unexerted until the stone begins to descend. According to the theory of conservation this unexerted power is not a reality which abides in the stone by virtue of its constitution, but a power that may be lost, and lost as quickly as it was gained. When the stone reaches the ground it possesses no energy beyond a portion of the heat generated by the collision. The advocates of the conservation hypothesis tell us that the potential energy of the stone at its maximum height is a power to do work. We inquire what work, and are gravely assured that the stone has power to fall, which it could not do when it rested upon the ground! Let us suppose that when the stone leaves my hand I was standing on a covered coal-pit two thousand feet deep, and that I remove the cover as the stone descends, if, as Mr. Brooke affirms, the Conservation of Energy is a fact, it follows that when the stone has fallen through twenty feet it will remain suspended over the pit. By its ascent it acquired power to fall only twenty feet, not two thousand and twenty feet. The metaphysicians, so much belaboured by Prof. Tait and other physicists, have ventured to think that the force of gravity has something to do with the fall of the stone. I have certainly found myself unable, even with the aid of the scientific imagination, to form an intelligible idea of the reality supposed to be symbolised by the term "potential energy." The theory of the Conservation of Energy as now maintained by physicists is opposed in several respects to the doctrine of the conservation of force as held by Faraday. Stewart, Brooke, and others teach most explicitly that energy is not only constantly changing its form, but always shifting about from one portion of matter to another. If I mistake not, Faraday asserts the very opposite respecting force. He seems to teach that each material particle, into whatever combinations it may enter, retains all its original forces. "A particle of oxygen," he says, "is ever a particle of oxygen."

Mr. Brooke makes other admissions which are as inconsistent with the truth of the doctrine of the conservation as the one I have examined. These, however, I must leave for the present. I feel that the most satisfactory reply to Mr. Brooke's strictures would be to quote here, with two trifling exceptions, the portion of my article which relates to the conservation of energy. Those exceptions I will now name. First, I withdraw what I have said respecting Mr. Brooke's view of the nature of latent heat. My sole reason for not in this connection quoting more was that I had assumed his perfect agreement with Prof. Tait. In this it seems I was wrong, since Mr. Brooke declares that he is unable to derive any definite idea from Prof. Tait's statement. I am sorry that Mr. Brooke should have supposed that the omission of the sentence named was due to a lack of literary honesty. I wonder that it did not occur to him that another and more charitable explanation was possible. Secondly, I was in error as to the weights employed by Dr. Joule in one of his experiments for determining the mechanical equivalent of heat. But this error relates merely to the form, not to the ultimate result of the experiment; and consequently in no way invalidates my reasoning. Holding, as I do, that forces are both conserved and correlated, I feel no difficulty whatever in accepting the facts established by Dr. Joule. He avoids speculation regarding the nature of force in itself, and deals exclusively with its manifestations. Thus, his discovery of "the mechanical equivalent of heat" is the discovery of a relation between two classes of effects.

There is one misrepresentation in Mr. Brooke's review of my article I must here point out. He says, "the reviewer thus quaintly expresses the relations of force, energy, and motion:—A given motion viewed as a cause is force, while the very same motion thought as an effect is energy." But this is not my doctrine. I am here dealing with the consequences of one of Mr. Justice Grove's assumptions, viz., that if we attempt to analyse

our conception of force, viewed as the cause of any perceived motion, we can get nothing beyond some antecedent motion. Mr. Brooke complains that the misapplication of the term "force" has led to great confusion in physics. His own statements are nevertheless unsatisfactory, if not contradictory. He accepts the definition of force given by Faraday. But this so-called definition by Faraday is not definition at all. It merely tells us what force does, not what force is. Mr. Brooke adds that the definition "may perhaps with advantage be thus amplified:—Force is a mutual action between the atoms or molecules of matter."

But these molecular actions or motions are the effects of force, but not force itself. In no instance whatever can force be resolved into molecular motion. Mr. Brooke says, "One finds oneself occasionally brought by circumstances into an unwelcome generalisation. Thus the reviewer, speaking of the supporters of 'conservation' in the lump, says 'they take it for granted that force is motion and nothing but motion.' This the writer entirely and absolutely denies." Will Mr. Brooke show that this denial is in harmony with his assertion that "force is a mutual action between the atoms or molecules of matter?" I cannot. My reasons for rejecting the assumptions on which the doctrine of the conservation of energy rests are not noticed by Mr. Brooke. These assumptions I have shown belong to false and exploded metaphysics. A false philosophy of causation, it is easy to prove, has greatly retarded the progress of science.

I perceive that Mr. Brooke has used for reference one of a small number of copies of my article printed for private circulation. Unfortunately the paging does not correspond with that of the review. Had I only anticipated the pleasure of an encounter with Mr. Brooke, I would gladly have sent him the review itself. As Mr. Brooke is aware of what passed at a very recent meeting of the Victoria Institute, I cannot longer withhold my name.

Salé, near Manchester, June 26

JOHN MOORE

#### Water Analysis

IN NATURE for June 27, 1872, Mr. Wanklyn directs attention to the facts that his paper on water analysis appeared in 1867, and that in 1868 he gave some absolute errors obtained with his process.

Mr. Wanklyn then proceeds to say:—"We have never said that distillation of albumin with alkaline permanganate converted the whole of the nitrogen of the albumin into ammonia. The assertion in your article is therefore untrue." Mr. Wanklyn's ideas of truth are probably peculiar, for if he will refer to his paper of June 20, 1867 (Chem. Soc. Jour. vol. v. N.S. p. 448), he will find the following:—"Direct experiments in which a known quantity of urea, gelatin, and albumin were taken, have shown that all the nitrogen in these substances is obtainable in the form of ammonia when they are subjected to the treatment about to be described, and has disclosed the very singular fact that boiling with a caustic alkali liberates one-third of the nitrogen, both of albumin and of gelatin, in the form of ammonia, and that a subsequent boiling with permanganate of potash liberates the other two-thirds."

Not a word is said in the paper about carrying on the permanganate treatment to dryness, and the only reference to such treatment is on page 450, where it is stated that boiling to dryness with potash alone causes the evolution of a "full third" of the nitrogen as ammonia.

THE WRITER OF THE ARTICLE

#### Scintillation

CAN any of your scientific correspondents tell me whether the following observation has been published, and, if so, where?

By very slight squinting, or (as suggested to me by a friend) by a slight pressure on one eye, we obtain two images of a star as viewed simultaneously from two stations a few inches apart. We made the experiment some nights ago, and could detect no relation whatever between the scintillations of the two. This seems to explain how little trace of the phenomenon remains when a telescope is used, for in that case we have a sort of integration performed over the whole aperture of the object-glass.

G. H.

#### To Entomologists

How often is it that the entomologist has to regret the want of his net? The rare butterfly, by some curious perversity, is